Answer to Rebecca Mott TC-2020-187

We thank Rebecca Mott for her comments. We provide here our responses to those comments and describe how we addressed them in the revised manuscript. The original reviewer comments are in normal black font while our answers appear in blue font.

This study introduces a modelling approach capturing processes which drive snow depth variability at the ridge and mountain range scale. While different models exist capturing snow redistribution processes at very small-scale, this study is able to efficiently model snow redistribution for large domains in a computationally efficient way. The paper is very well written and results are well presented. State-of-the art models and methods are combined to improve the spatial variability of snow depths at the ridge scale. Strengths and limitations of the model approaches are well discussed. Modelled snow redistribution was verified against ALS and SNETINEL-2 data at the end of the winter season. In my opinion a comparison with SNETINEL-2 data covering only the accumulation season would better represent snow distribution affected by snow redistribution processes (without contribution of snow melt). It would also be interesting to see how process representation affects the spatio-temporal snow dynamics. Please find my minor comments below.

Our response to this general comment is detailed below in our answers to the specific comments.

Comments:

Abstract:
The abstract is well-written and concise. The new wind downscaling strategy is mentioned in the abstract. I would recommend to add one sentence description of this method to abstract to give the reader a rough idea how the method works.
We rewrote the sentence describing the wind downscaling in the abstract to give more details about the method: "*In particular, a new wind downscaling strategy uses pre-computed wind fields from a mass-conserving wind model at 50-m resolution to perturb the meso-scale HRDPS wind and to account for the influence of topographic features on wind direction and speed*."

I recommend to better highlight the multi-scale approach by more clearly acknowledging the combination of regional-scale weather data and downscaling techniques allowing for snow redistribution modelling.
The multi-scale approach is now highlighted at the beginning of the abstract after the description of the main processes causing the spatial variability in snow accumulation and ablation:
"*The multi-scale approach combines atmospheric data from a numerical weather prediction system at km-scale with process-based downscaling techniques to drive the Canadian Hydrological Model (CHM) at spatial resolutions allowing for explicit snow redistribution modelling.*"

Only blowing snow is mentioned in the abstract. As you also calculate drifting snow via saltation this process should also be added;
We prefer the term blowing snow for the whole saltation and suspension transport, rather than drifting snow which is not well defined. This is now clarified in the abstract and the introduction.

Introduction:
P2, 44: I would recommend to add the reference Schlögl et al. 2018 for heat advection Processes
A good suggestion. Reference now added to the text in the revised version of the manuscript.

P3, L1: I am not convinced that 200 m resolution can be called a snow drift permitting scale, but this is open for discussion;
We mention the resolution of 200 m in the introduction here since this resolution has been used in two studies by Bernhardt et al. (2009 and 2010) focusing on high-resolution modelling of wind-induced snow transport in alpine terrain. Bernhardt et al (2010) compared two resolutions (200 m and 30 m) and showed that the simulated snow redistribution was quite sensitive to the choice of the resolution with an

overestimation of snow transport at 200 m. This lower resolution corresponds to the upper range of snowdrift permitting scales (defined as the range of resolutions that requires the activation of horizontal snow redistribution between computational element). A proper treatment of blowing snow at this resolution requires accounting for subgrid processes which are missing in the main models simulating blowing snow (as discussed in Sect 4.4). The resolution of 200 m used in Bernhardt et al (2010) is now explicitly mentioned at the end of page 3 when describing snowdrift-permitting models that can run over entire snow seasons.

P 3, p89: you could add here the possibility of modelling preferential deposition in atmospheric models;
Preferential deposition is now mentioned in the introduction as follows:
"These advanced models can be used for detailed studies such as the feedbacks between blowing snow sublimation and the atmosphere (Groot Zwaaftink et al., 2011) or the processes driving the variability of snow accumulation during a snowfall event, including preferential deposition of snowfall (Lehning et al., 2008; Mott et al., 2010; Vionnet et al., 2017)."

P5: please discuss the effect of the number of layers on the availability of erodible snow. Please add more details on whether the model distinguishes between hard and soft snow and which characteristics determine the erodibility of snow – e.g. wetness of snow?
PBSM-3D uses the formulation of Li and Pomeroy (1997) to compute the threshold wind speed for blowing snow. This formulation is based on a large, multiyear field dataset and depend on air temperature and snowpack presence. This approach is also used in the CRHM model (Pomeroy et al., 2007) and performs well for blowing snow simulation studies in the Canadian Rockies (e.g. MacDonald et al, 2010) and elsewhere, but is not coupled to the snow properties simulated by Snobal such as density and liquid water content.
The text of the revised manuscript has been modified has followed:

- In Sect. 2.2 describing CHM: "*It deploys the parameterization of Li and Pomeroy (1997) to determine the threshold wind speed as a function of air temperature and* ***snow presence. It is not coupled to the properties of surface snow (e.g. density, liquid water content) simulated by Snobal (see Sect 4.4 for a discussion on the limitation of this approach)."***
- In Sect. 4.4 discussing the limitations of the model: "*In addition, CHM uses the formulations for the threshold friction velocity of Li and Pomeroy (1997) that only depend on snow presence, and air temperature****. Though based on a large multi-year observational dataset, such parameterization is empirical and*** *does not also account for the effect of snow fragmentation during blowing snow events (Comola et al., 2017) which may lead to an underestimation of the threshold friction velocity and an overestimation of blowing snow occurrence in alpine terrain (Vionnet et al., 2013).* ***CHM may benefit from the inclusion of a more physically based snow transport routine in the future.****"*

P6, L179: is preferential deposition calculated as part of the suspension layer?
Preferential deposition of snowfall is not directly represented in CHM, apart from how it is addressed in the GEM atmospheric model microphysics scheme as part of snowfall calculations. In CHM, new snowfall is directly added at the top of the snowpack in Snobal and does not initially interact with blowing snow in the suspension layer. Instead, if the wind speed is larger than the threshold wind speed for snow transport, new snowfall is redistributed through both saltation and suspension blowing snow layers. The absence of explicit simulation of preferential deposition in CHM was already discussed in Sect 4.4. We added a sentence in Sect. 2.2.2 in the revised manuscript:
"Snowfall over complex terrain is calculated by GEM according to its microphysics scheme (Milbrandt et al., 2016). CHM does not simulate explicitly preferential deposition of snowfall (Lehning et al., 2008; Mott et al., 2018). New snow is added to the surface layer in Snobal and, if wind speeds exceed the threshold wind speed, it is transported in the saltation and suspension blowing snow layers by PBSM-3D"

P 6, L 180: here you could also add the effect of snow redistribution by avalanches on glaciers or ice fields (Mott et al., 2019)
This reference has been added to the text as follows:

*"In steep alpine terrain, gravitational snow transport strongly affects the spatial variability of the snowpack (e.g. Sommer et al., 2015), **the mass balance of glaciers** (Mott et al., 2019) and modifies the runoff behaviour of alpine basins (Warscher et al. 2013)."*

P8, L243: why has WindNinja used a bare ground instead of a smoother snow-covered ground? Is there any possibility to initialize WindNinja with a measured snow distribution?

In the wind simulations used in this paper, WindNinja has been initiated with a homogenous surface with a constant aerodynamic roughness of 0.01m. This value is the same as the one used to represent a snow-covered topography in alpine terrain with the ARPS atmospheric model in Mott et al. (2010) and Mott and Lehning (2010). In its present software version, WindNinja cannot be initialized with a measured snow distribution that would locally perturb the snow/atmosphere energy and momentum exchanges.

The following sentence has been modified in Sect. 2.2.4:

*"WindNinja used a spatially constant roughness length ($z_0 = 0.01$ m) **representative of snow-covered terrain in alpine topography** (Mott et al., 2010; Mott and Lehning, 2010) and vegetation effects …"*

P8, L 178: is 5 m enough to account for suspension plumes which can be much higher than 5 m?? In my opinion this arbitrary model height might be part of the discussion;

We selected 5 m for PBSM-3D since this height is usually sufficient to capture most of the mass transported in the suspension layer (see Figure 1 below) due to the strong vertical gradient of blowing snow concentration close to the surface. In steady state, 10-m wind speeds above 15 m s^{-1} are required to obtain a contribution larger than 10% for the part of the suspension layer above 5 m (Fig. 1) This contribution raises quickly above 15 m s^{-1}. However, the steady-state approximation implies fetches greater than 300 m that are rarely encountered in alpine terrain so that transport rates in alpine terrain are usually lower than the steady state approximation for the same wind speed (Naaim Bouvet et al., 2010). Therefore, we estimate that a height of 5-m is sufficient to capture most of the mass of snow transported by the wind over slopes exposed to the wind in alpine terrain.

On the other hand, we agree with the reviewer that such configuration may not fully capture suspension plumes, especially those forming above the leeward side of crest lines due to a massive advection of snow particles in the free atmosphere. In CHM, all the snow in the suspension layer that is transported across a crest remains a maximum height of 5 m above the ground and will eventually be deposited due to lower wind speed on the leeside of the crest. Such behavior is unrealistic and could potentially be improved by assuming a thicker suspension layer in CHM. A proper modelling of suspension plumes would ultimately require a three-dimensional wind field and calculation of flow separation in the lee of the crest.

In the revised manuscript we point out the limitation associated with the 5-m thickness for the modeling of snow plumes above alpine ridges:

"Finally, CHM uses a thickness of 5-m for the suspension layer (Marsh et al., 2020a). This is sufficient to capture most of the mass transported in alpine terrain over slopes exposed to wind with limited fetches (MacDonald et al., 2010; Naaim Bouvet et al, 2010) but it cannot simulate the formation of snow plumes at crest lines. Mass loss due to the advection of blown snow particles to atmospheric layers and subsequent sublimation are likely underestimated by CHM."

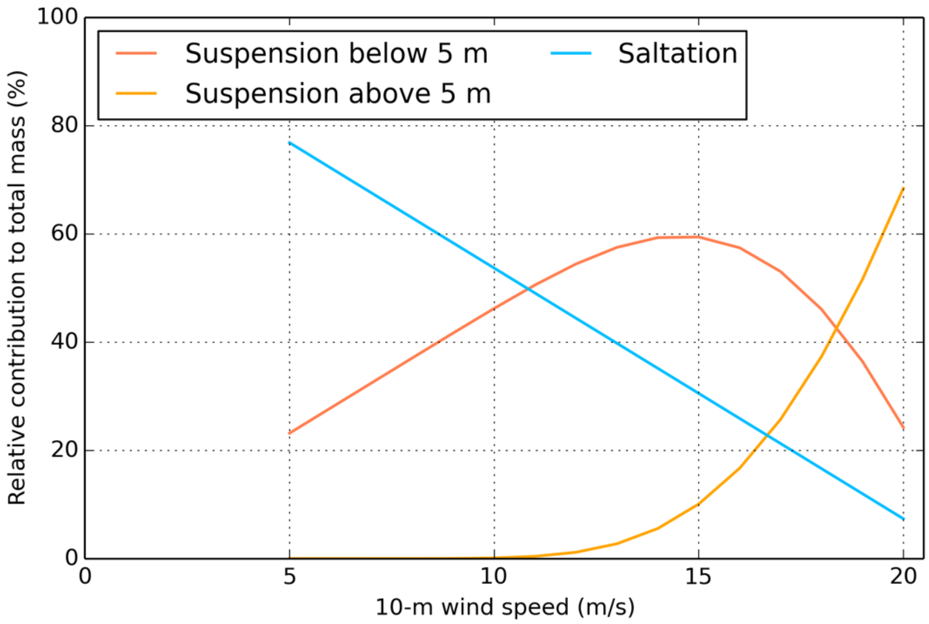

Figure 1: Relative contribution of the saltation and suspension layers to the total transported mass in steady state. For the suspension layer, the contributions below and above 5 m are considered separately. The mass concentration in the saltation and suspension layers were obtained from Pomeroy and Male (1992) assuming a roughness length of 0.01 m, a threshold wind speed of 0.25 m s^{-1} and a constant settling velocity of 0.5 m s^{-1}.

P 8: how sensitive is the blowing and drifting snow model to the model resolution? Why was the model resolution set to 50 m and not higher to better capture saltation but also to better resolve the wind field? Due to computational resources?

These are pertinent questions raised by the reviewer. We did not quantify the impact of mesh resolution on simulated snow redistribution in our study. In the paper describing PBSM-3D, however, Marsh et al (2020) show that a coarse discretization averaged out variability and removed the small-scale variability in areas of the simulation domain covered by large triangles (area of 300 m^2 for the coarse triangles VS 3 m^2 for the high- and fixed resolution reference mesh). We expect a similar sensitivity in our simulation domain and a higher mesh resolution would certainly reduce the overestimation of snow transport from windward to leeward slopes as reported in Mott and Lehning (2010) using a different snow redistribution model. A minimal triangle size of 50-m was used in our study to reach snowdrift-permitting scales and yet achieve reasonable computational time with CHM (less than 5h on 32 processors for the whole simulation). This 50-m resolution could be adopted for future operational basin-scale snowpack simulations in the Canadian Rockies.

The resolution of Wind Ninja has been set to 50 m to match the minimal triangle size and was not imposed by computational resources since the wind library is pre-computed before the CHM simulations. In our case, the maximal resolution of WindNinja could have been 30 m, corresponding to the original resolution of SRTM DEM used in this study.

P 11, L325: please change Grunewald to Grünewald

Changed

P 14, L418: strong, spatial —- delete the comma

Corrected

P 488-489: I do not understand this sentence

This sentence has been rephrased as follows:

“These results are consistent with the overestimation of gravitational snow redistribution to lower elevation and the erroneous location of avalanche deposits observed on Fig. 6b.”

P 17, L 520 – again. Is there a reason why the simulations were limited to 50 m? I thought that it is the advantage of the meshed grid to locally allow for very high resolutions. Especially, at the ridges higher resolution could have a large effect
The choice of the 50-m resolution was explained in our answer to a previous question.

P 17: L 539: mass-conserving
Corrected.

P 17: What is the contribution of snow melt to final snow depth pattern observed by SNETINEL-2 data and ALS in late April. The SENTINEL-2 data (Figure 10) show the snow persistence index SP at the end of winter. There are some slopes with very low SP values where I could imagine that lateral snow redistribution processes are of minor importance for the snow distribution as these might be more affected by melt. I recommend to additionally use mid-winter SP values which would better reflect the contribution of snow redistribution processes. A comparison of SP values at different stages of the winter would be highly interesting to reflect spatio-temporal dynamics.

The snow depth measured was measured by ALS on 27th April 2018. This date corresponds approximately to the peak snow accumulation over the domain (see Fig. 2 below).

We agree that the Sentinel-2 SP index reflects the spatial heterogeneities of both ablation and accumulation processes. In our paper, however, we compared different model experiments which only differed in their representation of the snow transport processes, while the melt algorithm remained unchanged. Differences in the simulated SP thus only result from the snow transport parameterization. The Sentinel-2 SP index was computed using images from 1st April 2018 to 31st August 2018. SP is suitable to identify areas that remain preferentially snow covered during the spring and the summer such as drifts or avalanche deposits due to large snow accumulation and areas with low ablations such Northern shaded areas. In wintertime, most of the pixels are covered by snow, even on windward slope exposed to wind-induced snow transport, so that a mid-winter SP (computed for example from 1st November) gives values close to 1 over most of the domain. To avoid this limitation, Wayand et al. (2018) proposed a snow absence (SA) index calculated from snow-free areas during the winter to identify areas of wind-erosion or avalanche source areas. However, the identification of such regions with SA is challenging since a few cm of snow are sufficient to cover them. For these reasons, mid-winter SP and SA were not considered in this study since they do not bring enough valuable information to evaluate CHM simulations. Overall, SP can only provide an integrative view of the spatiotemporal dynamics since it is influenced by variability in both snow ablation and accumulation (Wayand et al. 2018). At the moment, only successive ALS measurements can provide information on spatio-temporal snow dynamics at the basin scale (e.g. Hedrick et al., 2018).

A sentence has been added in Sect 4.2 to highlight the importance of accounting for variable insolation effects when using the snow persistence index to evaluate snow redistribution models:
"As illustrated by Wayand et al. (2018), the snow persistence index is influenced by variability in both snow accumulation and ablation, so that this index can only be used to evaluate snow redistribution

models if variable insolation effects are also simulated. This is the case in the simulations presented in this paper (Sect 2.2.5).”

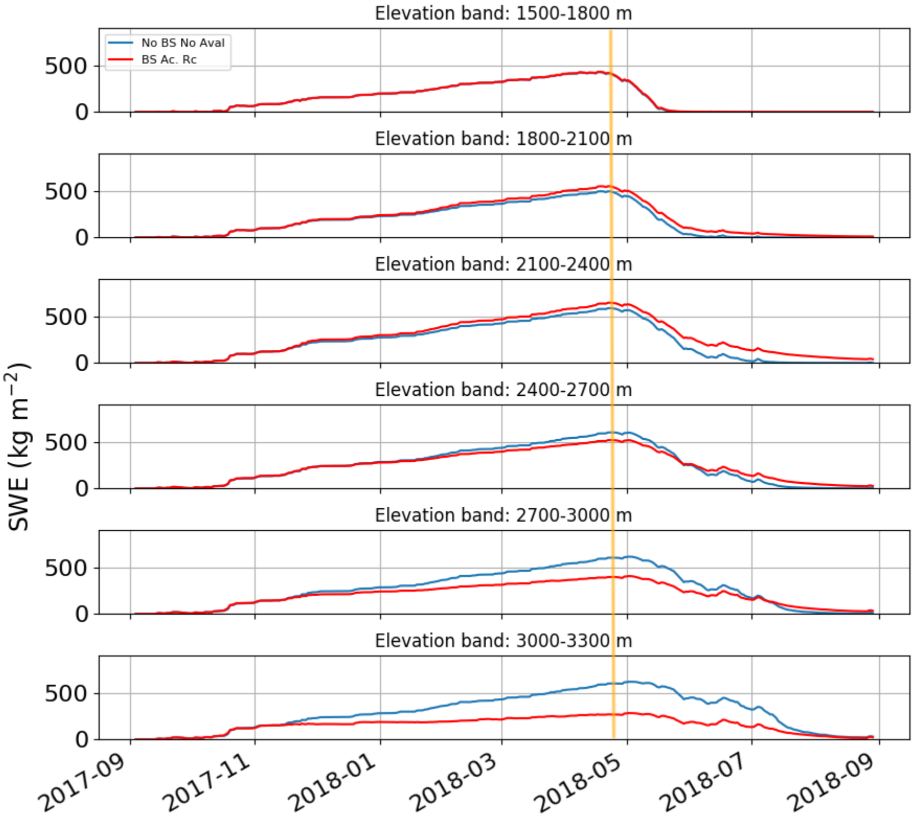

Figure 2: Temporal evolution of SWE in open terrain per elevation band for two CHM experiment. The vertical orange line shows the date when the ALS winter scan was collected (27 April 2018).

P 19: please also discuss the uncertainty due to the constant transfer function value fdown of 0.25. I could imagine that this value changes in downwind distance of the ridge and might be a function of wind speed and atmospheric stability.

The constant value of 0.25 used in this study in based on the initial developments of Winstral et al. (2009). The recent study of Menke et al. (2019) has measured the ratio, R, between the maximum wind speed in recirculation zones and the wind speed in the inflow at the crest. This ratio is similar to f_{down} used in our downscaling method to reduce the wind speed in areas prone to flow recirculation. Figure 10 in Menke et al. (2019) shows how R depends on the Richardson number (used to quantify the atmospheric stability). Their results show that R typically ranges between 0.1 and 0.5 for unstable atmospheric conditions and between 0.05 and 0.35 for stable atmospheric conditions. R tends to decrease with increasing stability in a stable atmosphere. As reported by Menke et al (2019), a ratio of less than 0.3 are observed for wind speed greater than 12 $m\ s^{-1}$, characterized by neutral or slightly stable atmospheric conditions. The study of Menke et al (2019) is mentioned in Sect. 4.3 in the revised version of the paper:

“A constant value of 0.25 is used for the transfer function in recirculation zones. This value falls within the range of values reported on Fig. 10 of Menke et al. (2019) for the ratio, R, between the maximum wind speed in recirculation flow and the inflow wind speed at the crest. Menke et al. (2019) found that R tends to decrease with increasing stability in a stable atmosphere and it presents values lower than 0.3 for inflow wind speed greater than 12 $m\ s^{-1}$*. This suggests that a dynamic value based on atmospheric stability could be used for the transfer function in recirculation zones.”*

Figures 6 and 10: poor visibility of grid lines;

The figures were modified to better show the grid lines.

P 21, L 669: In my opinion subgrid topographic effects primarily affect the local flow field which then affect snow redistribution.
We fully agree with the reviewer and modified the conclusion accordingly:
"This is potentially due to the absence of subgrid topographic effects ***in the driving wind field and in the snow transport equations*** *in CHM."*

P 22, L 678: high-resolution observations of what?
Thanks for noticing it. In the revised version of the manuscript we use: "*high-resolution observations such as ALS snow depth or Sentinel-2 snow cover."*

Suggested reference:
Schlögl, S., Lehning, M., Fierz, C., & Mott, R. (2018). Representation of horizontal transport processes in snowmelt modeling by applying a footprint approach. Frontiers in Earth Science, 6, 120 (18 pp.). https://doi.org/10.3389/feart.2018.00120

References

Menke, R., Vasiljević, N., Mann, J., and Lundquist, J. K.: Characterization of flow recirculation zones at the Perdigão site using multi-lidar measurements, Atmos. Chem. Phys., 19, 2713–2723, https://doi.org/10.5194/acp-19-2713-2019, 2019.